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Teilhard de Chardin's Biological Ideas

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TEILHARD DE CHARDIN'S BIOLOGICAL IDEAS

Dr. Alexander Wolsky

I. Introduction

ANALYSTS OF Teilhard de Chardin's intellectual legacy today generally agree that his great evolutionary vision, as outlined in *The Phenomenon of Man* and other writings, is not scientific in the strict sense. In fact, many who came to his defense find that the main cause of the many vicious attacks against his views was the misunderstanding created by his insistence that *The Phenomenon of Man* should be read as a scientific treatise, while in reality it was a mixture of science, philosophy and theology — with a good deal of poetry and mysticism in it.[1]

Nevertheless, his ideas have a solid scientific foundation, as the warm Introduction to the English edition to the *Phenomenon* by Julian Huxley, the great authority on organic evolution, testifies. Teilhard's complex personality and many-sided scholarship, versatility in such diverse subjects as philosophy and theology on the one hand and natural sciences, in particular geology and paleontology (including paleobiology) on the other, make it very difficult to segregate the various components of his thought and find the purely scientific elements in them.

In spite of these difficulties, the aim of this essay is to investigate this one particular aspect of the work of Teilhard, the scientific, or more specifically the biological, background of his thinking, and his ideas about the mechanism of organic evolution. (Emphasis is placed here on the word *organic*, i.e., the evolution of *living* beings, since Teilhard did not confine his speculations to this phenomenon, the emergence of *Homo sapiens* from the primeval slime, but started with cosmogony, the origin of matter and energy and other pre-biotic aspects of evolution. These will not be discussed here.)

It is perhaps this aspect of Teilhard's work, his concept of the

[1] A thorough analysis of the methodology of *The Phenomenon of Man* was made by Fr. R. J. O'Connell, S.J. in a recent lecture, the abstract of which was published in the *Newsletter* of the American Teilhard Association (O'Connell 1978). He points out that Teilhard's term *mémoire* is not accurately translated by the term *treatise*, which suggests an adherence to "all the accepted canons of present-day scientific methodology."

mechanism of evolution of living organisms which was most severely criticized, not only by those who obviously *wanted* to find fault in him because his whole ideology was so alien and indeed repugnant to them (two Nobel Prize-winning scientists, Peter Medawar and Jacques Monod attacked him, independently but equally rudely and viciously, *not* on a scientific but on an ideological basis[2]), but also by those who otherwise viewed his philosophy with sympathy. Apart from specific technical criticism of his paleontological work and his interpretation of the fossil record unearthed by him and by others, which again came both from ideological adversaries like G. G. Simpson (1960, 1973) and sympathizers like G. Vanderbroek (1969) of Louvain University, the more general scientific criticism concentrated on three main objections to his biological ideas: (1) his espousing the idea of orthogenesis (the concept that evolution proceeds in "straight lines" as if driven by some "inner force" rather than molded by environmental conditions), (2) his inclination to a Lamarckian interpretation of the mechanism of evolution ("inheritance of acquired characteristics" and related ideas) and (3) his neglect of the Darwinian concept of evolution (evolution by natural selection and "the preservation of favoured races in the struggle for life" — as Darwin himself put it in the subtitle of the *Origin*) which, reconciled with the Mendelian principles of heredity, has become in recent years the dominant idea in the thinking of the theorists of evolution and is often considered, as the "synthetic theory," the definitive answer to the problem of the mode of evolution.

In the following an attempt will be made to analyze these three main points of Teilhard's biological ideas, *sine ira et studio*. But before this attempt is made it must be mentioned here that an excellent book-size study exists already from E. O. Dodson, professor of biology of Ottawa University and senior author of the well-known textbook *Evolution: Process and Product* (Dodson and Dodson, 1976). This study, *The Phenomenon of Man Revisited — A Biological Viewpoint on Teilhard de Chardin* is now being prepared for publication, and Professor Dodson very kindly sent a photocopy of the manuscript to the author, an immense help in sorting out the thoughts and points presented here. Sincere thanks are due to Professor Dodson for his generosity. Although some of his views, as expressed both in his textbook and his unpublished manuscript, are not fully shared by the author, there is full agreement in the general approach and final conclusions, which fact is here gladly acknowledged.

[2]Attention is drawn especially to Monod's (1970) impassioned and almost irrational preaching against the seductions of metaphysics and his militant denunciations of Teilhard's "animism," as he called it.

II. Orthogenesis

Many students of the gradual emergence of living beings in the course of evolution were struck by the fact that this process seems to follow in many instances straight lines, as if certain trends — almost "ideas" or "inventions" — were developed gradually, step by step. Once a new structure or functional "design" appears, it often seems to advance further in one specific direction. Whether it is the wing pattern of butterflies or the antlers of deers, it will gradually increase — or decrease — in number, bulk, or complexity in consecutively appearing species. Since the early studies of the German zoologist Theodor Eimer, who studied butterfly wings and introduced the term *orthogenesis* for the idea of straight evolutionary lines, and the Belgian paleontologist Louis Dollo, who used mostly fossil vertebrates (and whose name is associated with the principle that the major evolutionary steps are irreversible — Dollo's law), there have been many distinguished biologists and paleontologists even in more recent times, contemporaries of Teilhard, who were deeply impressed by the seemingly one-directional nature of evolutionary processes and who devoted much thought to its causes and consequences. The American paleontologists H. F. Osborn, who introduced the term *aristogenesis* (see Osborn 1934), W. K. Gregory (1936), the German paleontologist O. H. Schindewolf (1936), zoologist R. Woltereck (1931), and many others have seriously considered the problem in a positive way in their studies. Teilhard knew these studies (as many orthogenetic concepts came from paleontology) and they must have had an impact on his thinking. He certainly was in very good company.

Teilhard applied the orthogenetic concept to the interpretation of his paleontological findings concerning a group of rather curious burrowing rodents ("mole rats") living in China (Teilhard 1942). He summarized his orthogenetic interpretation of the evolution of this group in a paper presented at a symposium in Paris in 1947 (Teilhard 1950), soon after his return from China where he had been practically a prisoner during the Japanese occupation in the war years. The mole rats are a difficult group to interpret and evaluate and opinions may differ about the significance of single facts concerning their phylogenesis.[3] So it is not

[3]The author has first-hand knowledge of such difficulties because the mentor of his doctoral work, Louis de Mehely, one-time professor of zoology at the University of Budapest, was an expert on a related group living in Eastern Europe ("ground dogs" in the vernacular). Mehely's monograph on the subject, *Species generis Spalax* (The Species of Ground-dogs from an Evolutionary and Taxonomic Viewpoint; 353 pages with 33 plates and 10 text-figures, Budapest, Hungarian Academy of Sciences, 1909) is still a standard reference work, although its details concerning the relations of the species to each other and to their assumed ancestor, *Prospalax priscus* were, like Teilhard's mole rat work, often debated and criticized. The author wishes to thank his friend, Dr. Olga Sebestyen (Hungarian Biological Research Institute, Tihany) for providing the bibliographical data of Mehely's monograph.

surprising that Teilhard's views were not left unchallenged, especially after he achieved posthumous fame. His great paleontologist adversary (and personal friend) G. G. Simpson was present at the Paris symposium of 1947 and already at that time expressed his objections (Simpson 1950). He then repeated them again in 1971 at another symposium in San Francisco which was devoted entirely to Teilhard's world view (Simpson 1973).

According to Teilhard, the evolution of the mole rats started with a rapid morphological differentiation of three distinct lines. Then these three lines evolved further in a parallel manner, more or less simultaneously. Three morphological characters appeared in this manner: (1) a change from rooted molar teeth to a rootless type, (2) a fusion of the cervical vertebrae and (3) increase in the overall body size.

According to Simpson's criticism all three of these generalizations have some flaws. The change from rooted to rootless molar teeth is not quite simultaneous in the three parallel lines, the fusion of the vertebrae of the neck has no intermediary forms (it does not exist in the early pliocene *Perisiphneus licenti* and occurs to the full extent in the later appearing species) and the increase in body size is not universal: the late pliocene species *Siphneus omegodon* is not larger than its presumed ancestor *Perisiphneus praetingi* or an even earlier Pontain species which Teilhard discovered.

Anyone who ever dealt with living beings scientifically knows the capricious nature of "living matter" and its tendency to escape generalizations, whether they concern morphological features, physiological functions or developmental processes. Most biological phenomena, especially those connected with evolution, can be grasped only statistically and individual fossil remnants are notoriously unsuited for such treatment. Exceptions, deviations will always turn up — in some groups more often than in others — and a particularly sharp eye and morphological "instinct" is needed to recognize general tendencies and "laws of nature" in a few fossilized bones. To make petty objections to such decipherings of general "groping" manifestations on the basis of some "imperfections" is quite irrelevant, especially in the context of such an occasion as the San Francisco symposium where the world view of a visionary thinker was to be evaluated. (It was even more irrelevant on such an occasion to bring up the fact that Teilhard did not use the "legal" scientific names of the mole rats, in accordance with the International Code of Zoological Nomenclature and should have called them *Myospalax* rather than *Siphneus*!).

But Simpson's main objection to Teilhard's interpretation of the

evolution of mole rats had to do with his adoption of the orthogenesis concept, even in the title of his paper at the Paris symposium. In righteously condemning Teilhard by declaring that he "ignored the consensus on this subject, did not understand theories current in his own lifetime, overlooked or brushed aside practically all evidence from them, and ostensibly based his own views on evidence that does not, in fact, logically support them" (Simpson 1973, page 100), Simpson clearly revealed that for him neo-Darwinism (the "synthetic theory") is the only acceptable explanation of the mechanism of evolution, a position which we shall discuss further below.

Obviously, neo-Darwinism is quite incompatible with the idea of orthogenesis or any other "internally determined" evolutionary force, and in this confrontation it was easy to show the supremacy of the neo-Darwinian concept. By 1971 it was indeed the "general consensus" that orthogenesis is in most cases a sort of "optical illusion," especially of paleontologists, based mainly on incomplete evidence of the fossil record. In earlier times one of the most often quoted examples of orthogenesis was the evolution of the horse (the "parade horse of evolution," as an early critic of evolutionary arguments, Albert Fleischmann of Erlangen University called it), more precisely the gradual reduction of the toes in the legs of the ancestors of today's species during the recent past of geological history, the Caenozoic era. Five toes were found in the legs of *Eohippus* (or *Hyracotherium*) of the early Eocene (the "sunrise" of the Caenozoic) and the somewhat later *Orohippus* of the mid-Eocene,[4] three toes in *Mesohippus* from the lower Oligocene, reduced side toes (not touching the ground) and enlarged mid-toe in the Miocene (*Merychippus*) and finally one single toe with rudimentary splinter bones on each side in the leg of our present-day horse (*Equuus*). (Most of the above names are today "illegal" or obsolete according to the International Code!).

It happens that it was Simpson himself who deflated the convincing power of this orthogenetic series by showing that there were side branches on the ancestral tree of the horse-like mammals: a separation of browsers from grazers about 25 million years ago (early Miocene), extinction of the former and shaping the morphological characteristics of the latter (such as height and the folded enamel pattern of teeth needed to grind the hard, silica-containing grasses of that time — hypsodonty, weight of hooves, size of body and especially size of the side toes) not by some inner pressure with predestined direction but through adaptation

[4]More correctly, three fully developed toes in the middle, flanked by two rudimentary splinter bones on the hind legs, four equal toes and one splint on the forelegs.

to environmental conditions and feeding habits which prevailed unchanged for millions of years, i.e., the natural selection of the fittest, leading eventually to our familiar horses (Simpson 1951). The side toes were gradually reduced and lost not because of an irrepressible inner urge to reduce them but because this was advantageous for running and jumping over obstacles while the browsers were stuck in the tall grass and eventually died out. This was the message of the "new synthesis" which became increasingly popular and generally accepted after the mid-1930s while other concepts, including orthogenesis, were gradually abandoned.

There are still a number of cases which some experts are tempted to interpret by assuming some sort of orthogenetic mechanism, like the progressive multiplication of ridges in the fossil shells of the snail genus *Paludina* in successive layers of sediments in the lower Pliocene, or the progressive proliferation of the antlers of deers, or the tusks of elephant-like mammals (Proboscidea), which in some cases becomes so excessive that it endangers the species' survival. Some regressive serial steps are equally impressive, like the gradual loss of the shell in the evolution of marine tortoises, the reduction of the bones of the jaw and the number of teeth in vertebrates (together with the general tendency to reduce the number of bones composing the skull). In other cases, where seemingly orthogenetic lines lead to disadvantageous excesses (oversize antlers or teeth) it is assumed that these might be unfortunate side effects of a genetic constitution which in other respects gave advantages to the organism (perhaps a hormonal mechanism). A non-orthogenetic explanation of such cases, based mainly on the concept of inborn differential growth rates for different parts of the body (allometry) is given in Dodson and Dodson (1976, pp. 335-339).

But the main reason for the sceptical attitude toward the idea of orthogenesis is that there is no material basis for it. If there would be some built-in force in the organisms to drive their transformations in any particular direction, this should manifest itself in the "molecule of life," the double helix of DNA (deoxyribose nucleic acid). But there is nothing in that molecular structure which would indicate the possibility of serial changes ("directed mutations" as it were) in any particular direction.[5]

Of course, the last word has not yet been spoken in this line of research or in any other scientific inquiry for that matter. As Jean Piaget, the recently deceased great psycho-biologist, says in one of his numerous

[5]There was a brief period when it seemed that "directed mutations" could be produced experimentally in the fruit fly *Drosophila* with temperature shocks (Jollos 1931), but these experiments could not be repeated by others (Plough and Ives 1935) and probably some experimental error, perhaps genetic impurity of the experimental stock, was involved in the original findings.

thought-provoking books (Piaget 1971, p. 123), "we must not forget the very interesting 'biochemical orthogenesis' pointed to by Florkin (1949)." And there are still many aspects of the molecular complexity of the DNA's genetic effect which have not yet been investigated. Over and above the "information content" encoded in the nucleotide sequences of the famous double helix, there might be further genetic information in the secondary or tertiary, three-dimensional configuration of the essentially one-dimensional (linear) structure of the DNA molecule. The latest studies concerning the supercoiling of the DNA molecule (Bauer et al. 1980) keep this possibility still open. But we must admit that even so, a revival of the idea of orthogenesis is rather unlikely. The concept is today more like a ghost which occasionally haunts the minds of biologists.

The idea was gradually abandoned not so much because it was proved erroneous but rather because there was less and less need to invoke this assumption to explain facts. Père Teilhard still used it in his paleontological writings thirty and forty years ago as he thought that this is what he called the *bonne explication* of his findings on the Chinese mole rats. But does his adherence to the orthogenesis concept in papers written in 1942 and 1947, when orthogenetic views were still widespread, alter the value of their general conclusions? Professor Dodson in his unpublished book points out that the established facts of those papers are just as valid, or perhaps even more convincing, with a neo-Darwinian than with an orthogenetic interpretation.

III. Lamarckism

Another ghost, which haunts the theorists of evolution even more disturbingly than the idea of orthogenesis is Lamarck's theory of the causes of transformation. This concept, formulated well before Darwin's theory of natural selection, emphasizes the well-known fact that environmental conditions have a direct, immediate effect on the structural details of the organism. The temperature and humidity, the amount and composition of the food available, the amount of light (especially in the case of plants) and many other factors will determine the size, the stature, the proportions, and various other details of the internal organization or external appearance of animals and plants alike. This well-known fact, however, is in itself not enough to explain major transformations of the species, the emergence of new forms or types unless we assume that the structural differences produced by environmental differences will be inherited by the offspring and can be thus increased from

generation to generation until they reach species, and even above-species dimensions. Lamarck took this assumption for granted and built his theory around it. There were other assumptions in the theory, some also based on readily observable facts, such as the enhancing effect of extensive use, and the reducing effect of neglect, on certain organs or mechanisms. (One must just think of the muscles of a weightlifter who is regularly "pumping iron," or the wasted legs of a sick person confined for long periods to bed or wheelchair). Other Lamarckian assumptions were more speculative, such as the idea that the organisms (especially higher animals) have some sort of inner urge, an unconscious feeling of their needs in a certain environmental situation (Lamarck used the expression *besoin* for this drive) and this urge will lead to the development of the remarkable adaptations of organisms to their environment which Lamarck quoted in his *Philosophie Zoologique* (1809), where the theory was laid down. One of the most often quoted examples is the giraffe, whose long neck is supposed to be the consequence of continuous stretching, for many generations, enabling the organism to reach sources of food — leaves of trees — not available to short-necked grazing species. Another example, a negative adaptation, is the existence of many species of animals, both vertebrates and invertebrates, which have no eyes or very rudimentary eyes, because their ancestors have lived for many thousands of generations in dark environments, in caves or in the deep sea.

All this was said exactly fifty years before Darwin published his *Origin of Species* in which he explained his theory of natural selection. The two theories peacefully coexisted for many years. Darwin accepted the Lamarckian mechanism as something almost self-evident, though he attributed to it a limited role in the shaping of species compared with the selective force of his "struggle for life." Only when the question of the mechanism of inheritance became a central issue around the turn of the century was the inheritance of acquired characters seriously challenged. Even before the triple rediscovery of Mendel's long neglected laws in 1900, the brilliant German zoologist August Weismann raised the question of what is inherited and what is not. In his thought-provoking book (Weismann 1885) he envisaged a strictly material basis for the transmission of hereditary characters from parents to offspring, though the exact mechanism remained unknown. He emphasized that the material of heredity, which he called idioplasm, or germ plasm, must be independent from, and even protected against the influences of the rest of the body, the *somatoplasm* or just *soma* in his terminology. Whatever the organism acquires during its lifetime, by nutrition, by prolonged usage (or neglect) or in any other way, will affect only the somatoplasm which

dies with the individual, while the idioplasm which will be transmitted to the offspring from generation to generation, and is thus in a biological sense immortal, will remain unaltered.

This concept harmonized well with the Mendelian laws of heredity, based on the concept of rigidly preformed corpuscular hereditary units, and after 1900 the new science of genetics (a term introduced by the early British Mendelist, William Bateson) soon became a strong support for Weismann's thesis that acquired characters are not (and cannot be) inherited and that, therefore, the whole Lamarckian concept of the origin of new species is untenable.[6] Weismann himself made some crude experiments to show this, by chopping off the tails of newborn mice in some twenty consecutive generations and observing that this had not the slightest effect on the length of the tail in the offspring.

Nevertheless, the Lamarckian concept, perhaps just because it seems superficially so convincing, continued to exert a strong influence on the thinking of biologists concerned with the problem of evolution. Attempts were made again and again to find some way out of the impasse and to prove that under certain circumstances certain "acquired characters" might be still inherited. McDougall's attempt (1927) to show that the capacity of learning (orientation in a labyrinth) increases in rats from generation to generation kept alive for a while the belief in some sort of Lamarckian inheritance of acquired characters. (G. B. Shaw believed that people learned easier to ride a bicycle if their parents were also bicyclists). Later, however, McDougall's experiments were discredited. More tragic was the outcome of the somewhat earlier attempts of the Viennese biologist Paul Kammerer to show that even visible morphological characters acquired in a lifetime can be inherited, such as the influence of the color of the substrate on the pigment pattern of the spotted salamander *Salamandra maculosa* or the effect of a certain light on the rudimentary eyes of the blind cave-dwelling newt *Proteus anguineus* and especially the influence of a forced aquatic environment on the formation of nuptial pads on the forelegs of the midwife toad, *Alytes obstetricans*, a character not existing in this species under normal circumstances. These experiments failed to convince the opponents of Lamarckism and when Kammerer was accused of deliberate deception in the latter case, he committed suicide in 1923. (His case is sympathetically reexamined in detail by Koestler 1971). Later, when Herman J. Muller and others demonstrated that X-rays can produce changes (mutations)

[6]Recent studies on the formation of primordial germ cells (i.e., the carriers of the "idioplasm") seem to contradict Weismann's concept of sharp separation of idioplasm and somatoplasm (cf Neiuwkoop and Sutasurya 1979), but this is still debatable.

in hereditary material, this was also interpreted by some as a proof that acquired characteristics can be inherited, but of course such radiation-induced mutations are certainly not the kind of "acquired characters" which the Lamarckian theory envisages. Somewhat similar experiments were made by the greatly respected German biologist Oscar Hertwig (and his children, Gunther and Paula), inducing developmental abnormalities in frog larvae which developed from eggs exposed to radium or X-rays. Hertwig considered these cases as proof that environmental effects are transmitted from the affected egg to all the cells which developed from it. Hertwig, who had a very strong influence on the thinking of biologists in Germany was an ardent "Larmackist" and an even more ardent adversary of Darwinism which he was fighting not only as a biological error but also as an ethical and social evil (Hertwig 1922).

Thus Lamarckism lingered on even during the first decades of this century, in spite of all the great successes of Mendelian genetics, associated mostly with the name of Thomas H. Morgan and his school at Columbia University in New York. (The crowning success was Morgan's Nobel Prize in 1933). Lamarckism has its adherents even today among biologists, especially in Lamarck's (and Teilhard's) homeland, France. It was pointed out recently, in connection with a book review (Ghiselin 1970, Pasteur 1971) that *The Origin of Species* had a rather cool reception on the other side of the English Channel. One opinion attributes this to the influence of the Church on science in France — a rather startling conjecture, knowing the cultural atmosphere of the time — while others relate it to a certain Anglophobia, or just presume that "the French" do not understand natural selection. Of course, these are crude generalizations.

While many of the "Lamarckian" trends have undoubtedly emotional roots, there are at least two lines of inquiry which have more serious Lamarckian implications.[7] One of these is related to the modern concept of molecular-biological interpretation of the genetic mechanism. The so-called "basic dogma" of molecular genetics, the DNA — RNA —

[7]After this essay was written, a paper appeared in the Proceedings of the National Academy of Sciences (U.S.), "Inheritance of acquired immunological tolerance to foreign histocompatibility antigens in mice" by R. M. Gorczynski and E. J.Steele (PNAS 77 (5):2871-2875, 1980) in which the authors show that "a specific acquired somatic characteristic in the immune system (tolerance to major histocompatibility antigens) induced in male mice shows a significant transmission to first- and second-generation offspring." This new line of "immunological Lamarckism" (more accurately a reconciliation of Lamarckian and neo-Darwinian concepts in immunology) may very well develop into a major "third trend" in the future exploration of the mechanism of evolution, over and above the currently accepted smug neo-Darwinism. It is discussed in more general terms in a short book by E. J. Steele, *Somatic Selection and Adaptive Evolution* (Williams-Wallace, Toronto 1979). The author is indebted to Dr. Steele for a reprint of his PNAS paper and information about his book.

Protein "trinity" envisages that the "message" of the gene molecule (DNA) is translated — with the help of an appropriate mediator (RNA) — into "action" by the synthesis of a corresponding protein molecule, the primary gene product, which acts usually as an enzyme or catalyst of a certain biochemical process. This system has a built-in self-regulating mechanism associated with it (the *operon* of Jacob and Monod) which determines, in a feedback fashion, when and where the "translation" process should be started or stopped. This concept of self-regulation of genes on the basis of feedback information obtained from their immediate inner environment seems to contradict Weismann's long-prevailing and generally accepted idea that the idioplasm is untouchable and protected from environmental influences. The *operon* concept indicates that it *is* affected by the environment, if not in its chemical composition or molecular architecture, at least in its function.

The other trend is the increasing attention paid to the role of the integrative-interpretative function of the nervous system in the evolution of higher forms, the possibility of a somewhat "Lamarckian" evolution of behavioral pattern of a population, transmitted through learning from generation to generation, may be considered a distinct possibility ("psycholamarckism"). There seems to exist a sort of "selection in reverse" when a population as a whole, or certain members of it, select their natural environment according to their behavioral preferences. The distinguished Oxford zoologist Alister Hardy has in recent years strongly emphasized "behavior as a selective force" (Hardy 1967).

It would be difficult to go into the details of these two non-Darwinian evolutionary concepts here (they are discussed more thoroughly in Wolsky and Wolsky 1976) but perhaps these brief remarks are sufficient to show that the basic Lamarckian idea — direct environmental influences on the individual organisms and through these on the evolution of the species — is still not quite ruled out as a possible explanatory principle for some aspects of the mechanism of evolution. "Lamarckism" is one of those ghosts which reappear from time to time and Père Teilhard was one of many distinguished scientists who were visited by it.

IV. Darwinism (The "Synthetic Theory")

The third and probably most serious objection to Teilhard's biological ideas was that they ignored the so-called synthetic theory of the mechanism of evolution which is often called, quite correctly, neo-Darwinism or just Darwinism. This concept is essentially a reconciliation of Darwin's original theory of organic evolution by means

of natural selection with Mendel's laws of inheritance which — although formulated in 1865 — were completely ignored until their rediscovery in 1900.

It is not without interest to compare the history of these two great biological concepts. Darwin's idea of natural selection was received immediately with great enthusiasm and dominated the thinking of biologists almost until the end of the nineteenth century. But then, just as the time when Mendel's originally ignored laws were rediscovered, its "popularity" among biologists decreased. While the science of heredity achieved great successes in the first two decades of our century (not only a confirmation of the general validity of the laws but also the demonstration of the material existence of Mendel's hypothetical units for the transmission of hereditary characters in the chromosomes of the cells), the increasingly controversial speculations about the course and forces of evolution, based mostly on the interpretation of morphological and biogeographical observations in the Darwinian manner were met with growing scepticism and mistrust. Thus one can conveniently divide the earlier history of the idea of evolution into a "romantic" and an "agnostic" period (Dodson and Dodson 1976), the former dominated by Darwinism, the later by Mendelism.

The changing fortunes of Darwinism and Mendelism are causally related as there were seemingly irreconcilable differences between Darwin's original concept and Mendel's principles. Darwin realized that evolution hinges on what he called "the strong forces of heredity" but had totally erroneous ideas about them. (His ideas were, in fact, the "consensus" of his time: the constant blending of hereditary characteristics of the parents in the offspring and thus an unlimited "dilution" of these, a steady drifting of an interbreeding population toward complete uniformity). Mendel's concept, on the other hand, was based on the assumption of the existence of distinct corpuscular units ("anlagen" or "factors" in his vocabulary, "genes" in modern terminology) for the transmission of hereditary characters from parents to offspring, one pair for each character — one from the father, one from the mother — and strict methods of their segregation, combination and reappearance in later generations in accordance with the laws of probability. This is the exact opposite of blending intermediary inheritance! Hereditary differences will not be "blended," "diluted," or "washed away"; they reappear again and again from generation to generation.

Only after some thirty years, in the 1930s, was it realized that the blending inheritance concept is not an essential part of Darwin's theory and his ideas about the origin of species can be very well reconciled with Mendel's principles of heredity. One must only assume that the heredi-

tary variations (now called mutations) within a species with which Mendel worked in his experiments (differences in the color of flowers, or seeds, stature of plants, etc.), can occur again and again in the species independently from earlier occurrences and then natural selection will decide which of these genetic variations are "good" (i.e., adaptive) for the species and will be preserved, while the "bad" (maladaptive) ones will be eliminated.

Thus, after the "romantic" and the "agnostic" period came the era of "synthesis" — an almost ideal case of the Hegelian concept of intellectual progress: thesis — antithesis — synthesis. The leading pioneer of this "modern synthesis" was Theodosius Dobzhansky, whose book *Genetics and the Origin of Species* (1937) had about the same influence on the future course of evolutionary studies as Darwin's *Origin of Species* on an earlier era. Building on such foundations as the statistical studies of J. B. S. Haldane, Sewall Wright and especially Ronald Fisher (1932), as well as on his own earlier studies (and similar studies of Chetverikoff, Dubinin, Timofeeff-Ressovsky and others) on the widespread distribution of genetic variations (mutations) in natural populations, he presented overwhelming evidence that the interaction of mutation and selection (with some auxiliary mechanisms, such as various types of isolation) can be indeed the efficient cause of the origin of new species. Some years later Julian Huxley summed up the essential points of the new theory and his book *Evolution: The Modern Synthesis* (1942) added greatly to the general acceptance of both the theory and its widely used name. (It is interesting to note that both Dobzhansky and Huxley were greatly impressed by the ideas of Teilhard de Chardin and obviously did not think that his neglect of the synthetic theory has significantly reduced the value of his evolutionary vision.)

The synthetic theory dominated, and to a certain extent still dominates the evolutionary landscape. (A new, fashionable variety of it is the "sociobiology" of E. O. Wilson (1975) which emphasizes that the "selfish genes" and natural selection determine the behavior of animals and man.) The influence was particularly clear at the time when the learned world celebrated the 100th anniversary of the publication of the *Origin of Species* in 1959. Père Teilhard was dead by that time, buried in the Jesuit cemetery at Hyde Park, so we shall never know what he would have said on that occasion when practically everybody actively involved in one way or another in the study of the great problems of life made some comment.

However, a little-known fact of Teilhard's life, recently analyzed in some detail, may throw some light on his views on the synthetic theory, the happy marriage of Darwinism and Mendelism. In 1937 Villanova

University (Philadelphia, Pa.) presented its Mendel Medal to Teilhard de Chardin who was by that time already well known for his paleontological work in China and his evolutionary views, disseminated "by diffusion" (never in print). (Villanova is a university of the Augustinians, the religious order to which Mendel belonged and takes special interest in maintaining the priest-scientist, or more generally the religion-science connection, epitomized in Mendel.) The 1937 award, bearing the name of Mendel, came, many years later, to the attention of the Director of the Mendel Museum in Brno, Czechoslovakia, which is located in the former Augustinian monastery and specializes in collecting documents and references related to Mendel all over the world. He, with a co-worker, undertook a study in depth of Teilhard's Mendel medal and its implications. They were naturally more interested in Mendel than in Teilhard and wanted to find out the medalist's views about the founder of modern genetics and how his own work was related to Mendel's. They wrote a report on their studies in the journal of the Mendel Museum, *Folia Mendeliana* which is very illuminating (Orel and Matalova 1976). They have obtained the text of Teilhard's acceptance speech, which was printed in the 1937 *Mendel Bulletin* of Villanova University, entitled "Spiritual Evolution" and reprinted it as Appendix to their article.[8] But the speech is not especially informative. It is very brief, less than two printed pages and contains only one sentence about Mendel. So the authors, by means of an intermediary, contacted Sir Julian Huxley whom they knew had written the introduction to the English translation of *The Phenomenon of Man* for the sake of its scientific readership and asked him if he knew more about Teilhard's views on Mendelism (and thus also on neo-Darwinism). Huxley in his letter, written in reply to this inquiry (also reproduced in the Orel-Matalova paper), says that in his opinion Teilhard "seems to have paid little attention to Mendel and the later developments of genetics." He finds only one reference to Mendel in *The Phenomenon of Man* (page 108 of the English edition), where Teilhard speaks of "the mutation we call Mendelian." Huxley calls this, and very rightly, a "very muddled reference" and puts two exclamation marks after his remark. But the full text of page 108 of the *Phenomenon* gives some information and explanation. Teilhard says there

> If it had been left to their resources alone, life would have spread and varied, but always on the same level. It would have been like an aeroplane which can taxi but not become airborne. It would have never taken off ... There seems to be no lack of examples, in the course of biological evolution, of transformations acting horizontally by pure crossing of characters. One example is the mutation we call Mendelian.

[8]It is also reprinted in the July 1980 Issue of the *NEWSLETTER* of the American Teilhard Association, *13* (1):9-10. This brief speech was given one year before Teilhard began to write *The Phenomenon of Man* and may be considered a preliminary verbalization of his basic ideas.

In this context the "muddled reference" makes some good sense. (Perhaps more accurately we should consider it not so much muddled as rather overcompressed; an attempt to find one single phrase for both the Mendelian mechanism of inheritance and the role of mutations, i.e., genetic variations, in it. But the expression *mutation* is certainly not "Mendelian." It was never used by Mendel or Darwin, although it was coined by Mendel's contemporary and correspondent Carl von Naegeli and used by him in a speculative sense, not related to genetics in the sense we use it today.)

But in spite of "muddling" the Mendelian concept, there is a very important point in the above-quoted passage. It shows Teilhard's view that genetic variation (mutation) alone will never produce evolution in the true sense. It will increase the bewildering multiplicity and diversity of forms, colors, sizes, and other structural (and indirectly functional) characters of living beings but will not produce *progress*. Yet, there *is* progress in evolution (or at least "improvement" or "adaptation") and this needs some other force besides mutation. This is in full agreement with the synthetic theory. The difference is that Teilhard (on the same page) considers this other force "the law of controlled complication ... known to biologists as orthogenesis" while the synthetic theory maintains that it is natural selection.

Admittedly, it is difficult to translate Teilhard's choice of words into terminology commonly used by English-speaking scientists. (One wonders how much of this is due to difficulties of translation in the linguistic sense and how much is caused by differences between the often metaphorical technical terms used today by scientists in the English-speaking and French-speaking world respectively. Even the French, incidentally, may have some difficulty in interpreting several of Teilhard's expressions in their own language.) In the above sentence *mechanism* might have been a better word than *law*, and a clear statement on the nature of this mechanism ("inner factors," "genetic constitution" in contrast to "environmental influence," or something like that) might have helped. But it is quite clear that Teilhard agrees at least halfway with the synthetic theory: mutation alone is not enough to account for evolution (as some of the early geneticists thought) and needs a counterpart, a second component of the evolutionary machine. For the Darwinians (and neo-Darwinians or synthetic theorists) this second force is natural selection; for Teilhard it is some unknown inner urge to achieve "betterment" (is it Lamarck's *besoin*?) or some predetermined genetic program.